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Dear Al-

I hadn't read Harriett's paper until you called it to my attention. From what I was able to gather from it last night, I don't think that it shifts the bearing of the transformation work on recombination in E. coli and more than in Drosophila. The pneumococcus transformations have every appearance of a cytoplasmic phenomenon, and are strikingly parallel to induced lysogenicity (See M. Williams-Smith, J. Hygiene, 46: 82-89 (1948).

Except for the heterozygotes (which by the way, Zelle has amply confirmed in a couple of hundred single-cell isolations), I might leave the matter as it stands on p. 521 of my paper in Genetics, Sept. 1947. Now I think that the transformation hypothesis would be quite untenable, except as the transforming "substances" may actually be gametes in the same sense as in Drosophila.

There were two points in Harriett's paper that struck me as needing clarification. The first is her evidence that the intertransformations of the subtype SIII-1 are not mediated through a susceptible R form, which seems not only vague but weak. The second concerns the heterozygosity of transformed SIII-N types. I have the impression that she did not test -N derived from SIII-1 under the action of TP from SIII-2, but only those derived under the action of TP from SIII-N. If this is correct, she has not ruled out the possibility that the "interaction" of TP-1 and TP-2 to give TRIN is merely due the mixture of the two factors, which far from being "allelic", are complementary. I think that the use of "allelism" in the discussion was unfortunate, because it has little meaning in a cytoplasmic system. But if anything, allelism means exclusive alternatives on the one hand, and non-complementariness on the other, so that on this basis, the -1 and -2 are definitely non-allelic.

The paper on the heterozygotes should be out momentarily in the April PNAS, but I really don't think that transformation has any special bearing on it. See you at Cinncinnati. We'll be at the Fountain Square Hotel.

Sincerely